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PITTSBURGH UNIV PA SCHOOL OF ENGINEERING
M L WILLIAMS ET AL. MAY 82 AFOSR-TR-82-1020

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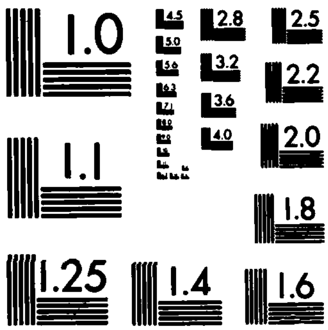
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This report is a supplement to the annual technical report on the Science of Fracture project dated 30 November 1981 to the Air Force Office of Scientific Research and completes the work conducted on Contract F49620-78-C-0101. The authors present their opinions as to the technical position of fracture mechanics within the framework of current military requirements, while simultaneously recognizing that this discipline is but one of several competing components of the overall technology base. The subject of

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mechanics has received a high level of attention. Through the concept of critical fracture toughness, advances in experimental stress analysis and mathematical analyses of failure have been translated into a practical and improved method for dealing with fracture. In the very basic research and conceptual sense, however, there has been, with few exceptions, relatively little progress. In the 1920's Griffith proposed the critical energy release rate criterion, and this concept has been the basis for most work in continuum fracture since that time. From the basic research point of view the field has been relatively stagnant for some time. A careful look at DOD funding over the past decade indicates the transition from basic research to applied research. The authors advocate renewed support of basic research for new concepts, and suggest five areas with potential for high payoff, including characterization of the 3-D singularity at a crack tip, the fracture phenomenon in composite materials, the connection between Weibull and Griffith failure theories, and design of materials to resist fracture. Additional high payoff basic and applied research areas include specific topics in adhesive fracture, defect implications in electro-optical systems, interdisciplinary approach to life prediction, probabilistic methods for structural integrity, and computational mechanics.

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May 1982

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THE STATE-OF-THE-ART IN THE MECHANICS OF FRACTURE

A Supplement to the Final Technical Report on the

Science of Fracture Project
Air Force Office of Scientific Research
Contract F49620-78-C-0101

by

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FOREWORD

This commentary is a supplement to the final technical report on the Science of Fracture project submitted on 30 November 1981 to the Air Force Office of Scientific Research and completes the work conducted on Contract F49620-78-C-0101. It is intended to describe the technical position of fracture mechanics within the framework of current military requirements, while simultaneously recognizing that this discipline is but one of several competing components of the overall technology base. These comments represent the opinions of the author and, while reflecting the result of innumerable inputs from personal discussions and impressions from the literature, must on the whole be subjective.

M. L. Williams

University of Pittsburgh
Pittsburgh, Pennsylvania

31 May 1982

SUMMARY

The subject of continuum fracture mechanics has, with a few major exceptions, been undergoing for some time a period of consolidation and refinement based upon past major contributions and break-throughs. The high level of attention accorded this subject has been based upon the desire for increasingly accurate assessments of structural integrity. Consequently a large number of practioners have been drawn to the subject, especially for design with metals. Here a technology transition has been demonstrated between the science of fracture and design methodology. Advances in experimental stress analysis and mathematical analyses of failure have, through the concept of critical fracture toughness, been translated into a practical and improved method for dealing with fracture. This major conceptual advance might be compared to the earlier quantitative improvement in failure analysis which resulted from treating non-uniformities in structures on the basis of geometric dependent stress concentration factors to be used in conjunction with an average ultimate stress.

In the very basic research and conceptual sense however, as completely distinct from the opportunities to exploit fracture technology in new engineering applications, e.g., with composite materials, for high rate penetration, and in adhesive fracture, there has been, with few exceptions, relatively little progress. If this proposition were granted, it would seem appropriate in allocating basic research support to carefully distinguish between research opportunities which had conceptual break-through potential from those applied research projects directed toward improvements in engineering applications.

As an illustration we might consider cumulative damage, say in the fatigue of aircraft materials. Clearly there is a high return on investment for more accurate life-cycle predictions as for example through increasingly complex and automated calculations. For the most part however, they are based upon long standing simple semi-empirical laws of crack growth and damage accumulation such as those due to Paris and Miner. One might inquire if a different balance between basic and applied research in this area would be more productive in the long run. Is it likely that a potential major break-through in the basic underlying laws could be found?

This commentary addresses certain aspects of the potential dichotomies, mainly with the purpose of identifying certain targets which could improve the basic understanding of the mechanics of fracture, as distinct from those areas of investment which, while indeed yielding a return, may have to compete with other disciplines in this latter regard. If, however, in our present research climate they fail to do so effectively compared to other worthy applied research projects, they may fail to be supported - however interesting they may be.

THE STATE OF THE ART IN THE MECHANICS OF FRACTURE

According to the Gamota report,[1] the Department of Defense supports research in 12 general areas, including Materials and Structures for which most support for studying the mechanics of fracture emanates. While there is reasonable flexibility in the Army Research Office, the Office of Naval Research, the Air Force Office of Scientific Research, and especially the Defense Advanced Research Projects Agency, the impact of the Mansfield Amendment requiring statements of relevance to the military missions has not been entirely eliminated. To provide a frame of reference the trend in total Basic Research (6.1) funding for Materials and Structures, along with data for the entire 12 areas and the important related categories of Exploratory Development (6.2) and Advanced Development (6.3) are given in the accompanying charts and tables.[1,2] (Figure numbers from the original references are retained.)

For FY 1981 the Department of Defense budget for basic research was \$652M of which \$85.9M (13.2%) was spent in Materials and Structures. (An additional \$98.8M + \$35.3M = \$134.1M was budgeted for Exploratory and Advanced Development for the field in essentially applied research and prototype hardware.) The large portion from DARPA (\$25M) was mainly in materials science with approximately one quarter of that budget being concentrated in non-destructive testing. Thus the Materials and Structures research support in the three Services was approximately \$61M.

A qualitative judgement of the author is that this latter figure of \$61M is split about 3:1 materials:structures research, thus leaving about \$15M per year for structures work, of which 10 percent is currently invested in continuum mechanics aspects of the mechanics of fracture excluding non-destructive testing and computational mechanics. If one were to use the histogram distribution of number of projects vs. project size reported by Gamota[1] (roughly 10 percent of the projects at \$150,000 and 90 percent at \$50,000) one would, on average, expect two to three large projects and 20 smaller ones. Recognizing the existence of support possibilities for fracture at NASA, NSF and other agencies, this estimate appears consistent with the present size of the research community in fracture.

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[1] Gamota, G., Basic Research Program, Department of Defense, Pentagon, 1 August 1980.

[2] Oliver, R.C., Retrospective Study of Selected DOD Materials and Structures R&D Programs, Phase II, Institute for Defense Analyses, Washington, D.C., Paper No. P-1555, June 1981.

By way of amplification, fracture investigations are generally approached from the micro-scale by materials scientists and engineers, and from the macro-scale by continuum mechanicists. In the former area, the early postulantes of Volterra on dislocations, Zener on micro-modelling, Frank-Reed on the dynamics of imperfections, and Cottrell on atomic strength, are characteristic of the major contributions which provided an understanding of how materials deformed, particularly in metals. For polymers, Rouse, for example, proposed a linked chain model with freely rotating joints to model the major features of a linear polymer, and Kelvin, Maxwell, and Weichert proposed various parallel and series springs and dashpots to simulate the basic stress-strain elements for viscoelastic materials. The advent of the electron microscope and then the scanning electron microscope are examples of major instrumentation impacts. The rapidly growing capability of computers has permitted more detailed point-by-point atomic simulation of aggregate atomic centers, even to the extent of inter-atomic force distributive laws which include the shadowing effect of force field interference from next-nearest neighbors. Generally speaking however, such advances in material science and engineering are not usually of direct value in assessing the macro-behavior of engineering components. They primarily affect the stress-strain law or constitutive relation, i.e., the equation of state relating stress, strain, time, and temperature.

On the other hand, one of the reasons that fracture analysis proves so complicated is that at the precise point of a crack, it is exactly the micro-behavior which controls fracture despite the overall attempt for practical design use to describe the crack and its potential growth as a macro-phenomenon. Hence the active collaboration of the materials community is required in a true interdisciplinary effort with continuum analysts.

Nevertheless, if the constitutive relation can be presumed known, it then becomes the job of the stress analyst to combine it with the equations of equilibrium and compatibility to predict the state of deformation and stress in a body of arbitrary shape and imposed loads. Historically this was done, primarily for isotropic, homogeneous, elastic bodies, and failure was predicted on the basis of the local stress exceeding the independently measured (average) failure stress by a ratio which became known as the stress concentration factor.

In the early 1920's however a new concept was introduced for analyzing failure. Beginning with the postulate of a pre-existing flaw or defect, Griffith, using the elasticity solution of Inglis, proposed that catastrophic fracture would occur when the energy consumed in causing the defect or crack to extend could no longer absorb the strain energy of deformation being released in the body as the crack extended. This concept is the basis for most work in continuum fracture over the last 60 years. During this time, most developments are applications refinements to the basic Griffith energy balance approach. A possible exception is the Weibull concept of statistical failure, i.e., fracture is more likely to occur in larger specimens, because the existence of a larger pre-existing flaw is more likely the larger the specimen.

As one might expect, with the passage of time it has become more and more difficult to make a large incremental improvement over the basic idea. Consequently, while the importance of accurate failure and life prediction has remained undiminished--indeed it has probably increased with the attempts to reduce life-cycle costs--the occurrance of major advances in the state-of-the-art has become less frequent. We have possibly the situation where the present fracture technology has become mature, while simultaneously attracting more practioners as its engineering importance for evaluating structural integrity, in conjunction with non-destructive examination (NDE), has grown.

From the basic research point of view, the field has been relatively stagnant for some time although that does not necessarily imply less sophisticated. Support for basic fracture research, including that from DOD, seems to have peaked circa 1975-80, and the results over the previous decades have been gradually transitioning into practice. In the DOD parlance, 6.1 has transitioned into 6.2 and 6.3 as have other previously matured technologies.

The purpose in making this point is not only to explain the tapering-off of basic fracture research support to the research community, but to suggest that the community and the DOD recognize this situation for what it is. Each should clearly distinguish between basic research for new concepts ("6.1"), and that for applied research in new applications ("6.2") and further refinements ("6.3"). It is believed that such a cleaner discrimination will aid materially in assessing the cost-effectiveness and return on investment in future research efforts, where perforce they must be compared to competing research proposals in other technology areas.

This argument is advanced for a further reason. Basic research tends to rely upon a general appeal for its support; applied research tends to justify itself, and be justified, on specific results. The former is somewhat like an insurance policy--a premium payment to explore a recognized important area and forestall technological surprise or provide technical break-throughs. In this sense then, one should be careful that the decreased funding for basic research in fracture should not completely disappear or be invested solely in new applications or unintentional "pot-boiling."

Careful attention to the distribution of basic research support, both in terms of numbers and size of project, throughout the Services is important if the basic insurance need is to be met.

Some Potential Areas for High Payoff

The first five areas are suggested candidates for basic research; the remainder have major basic research components but would benefit from applied research collaboration.

1. Character of the Three-Dimensional Singularity at Crack Tip

The basic Griffith work and subsequent contributions are based upon stress analysis incorporating a two-dimensional stress singularity for plane stress or plane strain. Actual plates incur a finite thickness effect ("shear lip") and probably can be characterized by a three-dimensional stress singularity which could substantially modify the Griffith result.

2. Analytically Remove the Singular Behavior at the Crack Tip

With any finite applied stress, the stress at the crack tip is mathematically infinite. Could this unreal phenomenon be removed by consideration of finite deformations and/or material properties, other than by invoking the Baranblatt-Dugdale hypothesis.

3. Fracture Pheonmenon in Composite Materials

Fracture in a composite is a complicated combination of cohesive and adhesive failure. Given a knowledge of either phenomenon separately, can a quantum jump over the "Law of Mixtures" rule be made from single fiber behavior in an infinite medium to that of randomly oriented multi-fibers in a finite medium? (Composite herein includes not only fiber-epoxy, metal matrix materials of construction, but also assemblies in fiber-optics and electronic VLIC.)

4. Connection between Weibull and Griffith Failure Theories

Both approaches are designed for the same purpose, although commonly used in different materials, i.e., metals and ceramics. Could they be shown to be consistent?

5. Design against Fracture

Given the propensity for a macro-material to crack as a result of initial failure of the micro-structure, what can be deduced about desirable changes in the material structure to postpone failure? This question implies the successful construction of the Interaction Matrix (Kelley-Williams, 1969) for quantitatively associating chemical structure and mechanical behavior.

6. Adhesive Fracture

Much has been found about the character of an elastic stress singularity in mixed-media with interfacial cracks. Because of its unusual oscillating mode it has been impossible to predict crack debonding direction. Similar problems as occur in cohesive fracture as to the material involved may or may not be obviously transferable to adhesive systems.

7. Defect Implications in Electro-Optical Systems

Defects in plastic or glass fiber optics are presently analyzed using a Weibull approach. Flaws which may occur during production of integrated circuits can lead to malfunction. Despite the effort on characterizing defects, the implications for structural, as opposed to optical and electronic, performance have not been fully assessed. In VLIC assemblies there are also debonding problems in the lead wires and layered strata assemblies. While this subject is in principle part of the general composite material area, its peculiar differences provide a fertile field for exploitation.

8. Life Prediction

Present fatigue calculations are semi-empirical and rely upon postulated slow crack growth laws. This area probably requires a truly interdisciplinary effort combining the best talents in continuum mechanics and materials science. Contemplated applications should include both metallic and polymeric materials. Improved accuracy in life-cycle prediction (and the accompanying costs) has a fantastic potential pay-off, but most methods of advanced sophistication have proved impractical.

9. Probabilistic Methods for Structural Integrity

This applied research subject recognizes the existence of practical design problems for structures which must be designed for random loading in intensity, frequency, and order. Different structural components can become critical at different times and places; thus appropriate modelling of both the loading spectrum, the structure, and the failure criterion must be included, as well as influences of hostile environments. Thus this topic extends beyond basic research in life prediction (for simple deterministic systems) and subsumes the existence of powerful computational aids.

10. Computational Mechanics

Increased computational capability has proved an invaluable asset over the years. It too has progressed in spurts, both for static and dynamic applications of the load--the latter at a wide variety of rates and for different equations of state. Continued cost-effective progress is still anticipated, but the key here is to apply the increased sophistication only to those problems where it will pay off. It may be observed that the U.S. computational versatility is a major reason for our present technological lead over the U.S.S.R. in fracture technology.

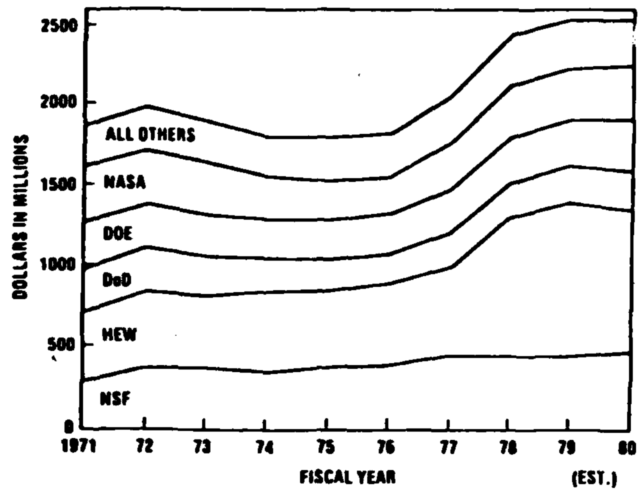

Figure A-2. Federal obligations for basic research by selected agency, in constant (1972) dollars, FY71 to FY80.

Type of RDT&E activity	Funding ($M)		Real growth (%)
	FY80	FY81	
Research	557.8	651.7	8[a]
Exploratory Development	1,702.3	2,072.5	13[b]
Advanced Development	2,783.4	3,094.8	3
Engineering Development	4,734.0	5,872.6	15
Management and Support	1,477.0	1,734.2	9
Operational Systems Development	2,262.0	3,059.6	25
Total	13,516.8	16,485.5	13

[a] *Real growth is 10% if reorientation of $12M from 6.1 to 6.2 in DARPA Nuclear Monitoring Program is accounted for.*
[b] *Real growth is 8% if reorientation of $72M from 6.3 to 6.2 in high energy lasers is accounted for.*

Figure A-3. RDT&E by activity type (millions of dollars).

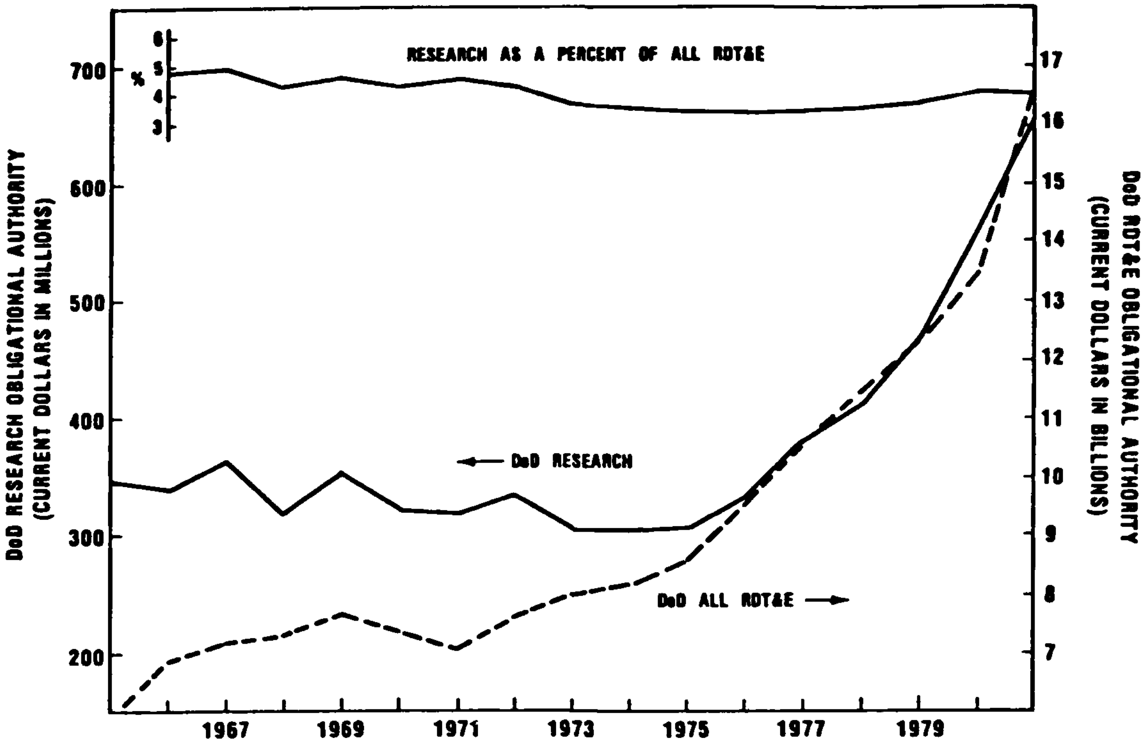

Figure A-4. Funding history of DoD research budget.

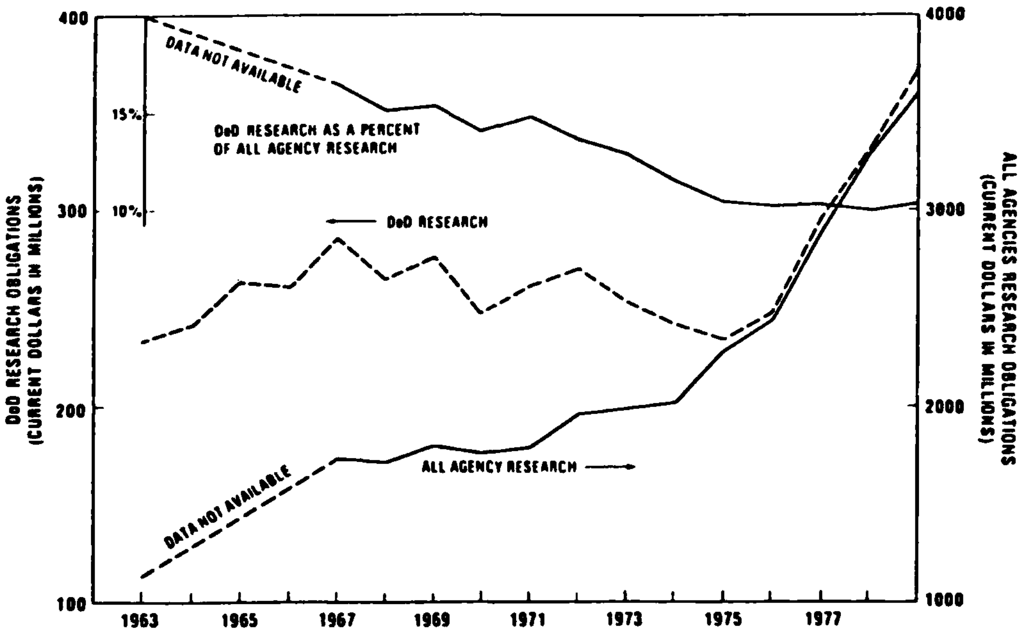

Figure A-6. Relation of DoD research budget to federal research budget.

Table A-1. Military Services Research (6.1) Funding (Obligational Authority)

Program element*	Funding ($M)		
	FY79	FY80	FY81
Army			
61101A (ILIR)	16.0	17.5	19.6
61102A (DRS)	98.1	113.7	137.3
subtotal	114.1	131.2	156.9
Navy			
61152N (ILIR)	18.1	19.1	20.7
61153N (DRS)	174.1	197.8	237.4
subtotal	192.2	216.9	258.1
Air Force			
61101F (ILIR)	8.2	9.0	10.2
61102F (DRS)	96.8	110.0	134.1
subtotal	105.0	119.0	144.3
DARPA			
61101E (DRS)	62.0	89.7	90.8
USUHS			
61101W (ILIR)	1.0	1.6	1.9
Total	474.3	558.4	652.0

**ILIR—In-House Laboratory Independent Research*
DRS—Defense Research Sciences
DARPA—Defense Advanced Research Projects Agency
USUHS—Uniformed Services University of the Health Sciences

Table A-3. Research (6.1) Performers

Programs*	Funding ($M)		
	FY79	FY80	FY81
Services			
In-house	175.8	191.0	216.6
Universities	178.8	210.2	263.5
Other contracts	56.7	65.9	79.2
Total	411.3	467.1	559.3
DARPA	62.0	89.7	90.8
USUHS	1.0	1.6	1.9
6.1 total	474.3	558.4	652.0

**DARPA—Defense Advanced Research Projects Agency*
USUHS—Uniformed Services University of the Health Sciences

Table A-5. DoD Program Funding (FY80 to FY81)

Disciplines*	Funding ($M)		Real increase (%)
	FY80	FY81	
Physics, Radiation Sciences, Astronomy, Astrophysics	77.2	91.3	9.5
Mechanics and Energy Conversion	58.3	69.7	10.7
Materials	49.7	59.3	10.5
Biological and Medical Sciences	49.0	58.5	10.5
Electronics	48.7	57.0	8.4
Oceanography	43.2	53.3	14.2
Chemistry	40.2	47.5	9.4
Mathematics and Computer Sciences	34.9	43.3	14.9
Atmospheric Sciences	20.0	24.0	11.1
Terrestrial Sciences	19.6	23.8	12.4
Behavioral and Social Sciences	17.4	21.0	11.7
Aeronautical Sciences	8.9	10.5	9.2
Subtotal	467.1	559.2	10.8
DARPA	89.7	90.8	—
USUHS	1.6	1.9	—
Total	558.4	652.0	8.1

**DARPA—Defense Advanced Research Projects Agency*
USUHS—Uniformed Services University of the Health Sciences

Table A-4. Allocation of Research Funding to Universities

Organization	Funding ($M)		
	FY79	FY80	FY81
Army	36.5	43.6	55.3
Navy	86.4	101.5	129.4
Air Force	55.9	65.1	78.8
DARPA*	17.5	19.6	18.8
Total	196.3	229.8	282.3

**DARPA—Defense Advanced Research Projects Agency*

Table A-6. Army Project (SPF) Assignments and Funding

Command/lab	SPF title and number	Funding ($M) FY80	Funding ($M) FY81
Program Element 61102A—Defense Research Sciences (DRS)			
Tank Automotive R&D Command (TARADCOM)	Res. in Vehicle Mobility, AF22	0.6	0.9
Army Materials & Mechanics Res. Center (AMMRC)	Materials and Mechanics, AH42	2.4	2.7
Armament R&D Command (ARRADCOM)/ Ballistic Research Lab (BRL)	Res. in Ballistics, AH43	6.8	7.4
Army Research Office (ARO)[b]	Scientific Problems with Military Applications, BH57	38.6	48.7

Task title and number	Funding ($M) FY80	Funding ($M) FY81
01—Geosciences	3.0	4.4
02—Biological Sciences	1.5	1.9
03—Communication Engineering and Electronics	8.4	9.9
04—Materials	5.0	6.5
05—Mathematics	5.2	6.3
06—Mechanics and Aeronautics	4.8	6.0
07—Physics	5.9	7.4
08—Chemistry	4.8	6.3
Total BH57	38.6	48.7

Command/lab	SPF title and number	Funding ($M) FY80	Funding ($M) FY81
Corps of Engineers (COE)/ Waterways Experiment Station (WES)	Soil and Rock Mechanics, AT22	0.6	0.6
COE/Engineering Res. Lab (ERL)	Structural Systems, AT23	0.6	0.7
COE/Cold Region Res. & Eng. Lab (CRREL)	Snow/Ice and Frozen Soil, AT24	1.4	1.7
Mainly non-Structures/Materials		62.7	74.6
Total 61102A		113.7	137.3
Program Element 61101A—In-House Laboratory Independent Research (ILIR)			
Total 61101A		17.5	19.6
Total Army 6.1		131.2	156.9

Table A-8. Navy Research Program

Project number and title	Funding ($M)	
	FY80	FY81
Program Element 61153N— Defense Research Sciences		
11—General Physics	24.4	28.6
12—Radiation Physics	3.1	3.4
13—Chemistry	11.7	14.0
14—Mathematical Sciences	15.3	18.9
21—Electronics	20.0	23.9
22—Materials	18.8	22.0
23—Mechanics	13.2	15.3
24—Energy Conversion	9.2	11.2
31—Oceanography	39.4	49.0
32—Terrestrial Sciences	11.5	14.3
33—Atmospheric Sciences	5.6	6.4
34—Astronomy and Astrophysics	4.0	4.3
41—Biological and Medical Sciences	15.3	18.1
42—Behavioral and Social Sciences	6.3	8.0
Total 61153N	197.8	237.4
Program Element 61152N— In-House Laboratory Independent Research		
Total 61152N	19.1	20.7
Total Navy 6.1	216.9	258.1

Table A-9. Air Force Research Program

Subelement number and title	Funding ($M)	
	FY80	FY81
Program Element 61102F— Defense Research Sciences		
2301—Physics	11.1	13.9
2303—Chemistry	10.9	13.4
2304—Mathematics	10.3	12.6
2305—Electronics	12.2	14.7
2306—Materials	16.2	18.9
2307—Mechanics	16.5	19.6
2308—Energy Conversion	8.3	10.6
2309—Terrestrial Sciences	1.9	2.4
2310—Atmospheric Sciences	7.2	8.5
2311—Astronomy & Astrophysics	4.5	5.2
2312—Biological & Medical	5.2	7.2
2313—Human Resources	5.7	7.1
Total 61102F	110.0	134.1
Program Element 61101F— In-House Laboratory Independent Research		
Total 61101F	9.0	10.2
Total Air Force 6.1	119.0	144.3

Table A-10. DARPA Research Program

Subelement title	Funding ($M)	
	FY80	FY81
Program Element 61101E— Defense Research Sciences		
Materials Sciences	19.6	24.1
Cybernetics Sciences	8.4	9.9
Computer and Communications Sciences	21.0	27.4
Unconventional Detection Research	6.2	7.3
Nuclear Test Verification	10.3	—
Charged Particle Beam	24.2	20.3
Geophysical Research	—	1.8
Total 61101E	89.7	90.8

Table 3.7* - Technology Base Support (6.1 + 6.2 + 6.3A) - $M

DOD Agency	Total RDT&E 6.1	Total RDT&E 6.2	Total RDT&E 6.3A	Total RDT&E Other	Materials/Structures 6.1	Materials/Structures 6.2	Materials/Structures 6.3A	Materials/Structures Total
Army	156.9				11.8 (7.5%)	16.1	11.8	39.7
Navy	258.1				23.9 (9.3%)	35.5	4.7	64.1
Air Force	144.3				25.2 (17.5%)	47.2	18.8	91.2
Services Sub-Total	559.3				60.9 (10.9%)			
DARPA	90.8				25.0 (27.5%)			
USUHS	1.9							
Sub-Total	652.0	2073	3095	10,667	85.9 (13.2%)	98.8 (4.8%)	35.3 (1.1%)	
Tech Base Total		5,820				220.0 (3.8%)		

*Extracted from Reference 2, by R. C. Oliver

END

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